Response to reviews of paper "Wave-optics uncertainty propagation and regression-based bias model in GNSS radio occultation bending angle retrievals" by M.E. Gorbunov and G. Kirchengast

Anonymous Referee #1

The authors present a new method for bias correcting GPS radio occultation (GPS-RO) measurements in the boundary layer. The boundary layer bias (BLB) model corrects a negative bending angle and refractivity bias in the boundary layer. The source of this bias is thought to be small scale fluctuations in the refractivity field. These are included in some simulations to demonstrate the effect. It is shown that the new bias model removes most of the BLB.
Given that one reasons GPS-RO has had an impact in operational NWP and climate re- analyses is that it can be assimilated without bias correction, introducing a bias correction scheme for this data is a significant step. I believe more is required to demonstrate that the small scale fluctuations are the ***main source of the bias****, and some discussion of the operational monitoring statistics from the NWP centres is also required.*

In fact, we do not believe small-scale fluctuations to be the main source of the bias. The small-scale fluctuation model is only used as a convenient structural model that allows finding objective candidates for bias predictors. The presented patterns in the scatter plots for the simulated and the real data indicate that the reality is more complicated than this model based on just one factor. We modify the corresponding text in the Introduction as follows:
"Although this model cannot be looked at as a complete explanation of the bias, is serves as a convenient structural model that allow exposing probable candidates for the role the objective characteristics of the signal received that may correlate with the bias. These characteristics will hereafter be referred to as predictors in the BLB model."

The following questions should be considered before publication.
Is the proposed bias correction model aimed at NWP or other applications?

We do this for an RO data processing chain with integrated uncertainty propagation where we follow the rules of the GUM,…; applications of the retrieved data are more cal/val of other observing systems, atmospheric process studies, climate applications. (And if you want to be provocative: if NWP guys use standard 1D/2D Abelian transform-/Simple raytracing-based forward operators, also they are better out with using our bias-corrected bending angle data rather than uncorrected ones, see above;)

Please provide spatial maps of the bending angle and refractivity biases, related to the profile information shown in Figures 1 and 2. How do the spatial maps of the simulated data in Figure 1 compare with "observed" COSMIC minus ECMWF bias maps? How do the observed bias maps correlate with parameters such as low cloud cover, and total column water? Can we be sure that the small-scale fluctuations are the main source of the bias?

We do not believe the small fluctuations to be the main source of the bias. Moreover, the proposed bias model does not use this model, as explained above. We provided spatial maps of the bias, Figures 3 and 8 (bias before and after application of our bias correction procedure). Discussion of the bias correlation with low cloud cover and total column water is beyond the scope of our paper, because our procedure is only based on predictors that derived from the objective characteristics of the signal received and observation geometry.

Figure 2. This is not consistent with standard operational monitoring at NWP centres. See, for example. http://www.romsaf.org/monitoring/index.php.
EG, GRAS measurements are biased positive with respect to both the ECMWF and Met Office models in the lower troposphere, and there are also differences for rising and setting data. This issue is complicated

because the forward models used in the NWP systems compute bending angles, but use a maximum gradient (~half ducting) in their computations for numerical reasons. Furthermore, they do not compute bending angles below ducting levels. Are similar restrictions used here? The point being that a relatively simple change like this, can have a significant impact on the sampling and subsequent biases, even changing the sign of the bias.

In our paper, we do not discuss nor use any special cut-off procedures used in the forward model. Note, we do not discuss any forward modeling here. On the other hand, we use the OCC package, which served as a prototype for the core ROPP package modules for RO data processing (wave optical and geometric optical inversion, statistical optimization, Abel inversion, dry temperature retrieval). These two packages were tested and found consistent. Our previous extensive study (Gorbunov et al., 2011) indicated that our results, including the negative bias of the refractivity retrievals, are in a good agreement with UCAR processing.

M. E. Gorbunov, A. V. Shmakov, S. S. Leroy, and K. B. Lauritsen, COSMIC radio occultation processing: Cross-center comparison and validation, Journal of Atmospheric and Oceanic Technology, 2011, V. 28, No. 6, 737–751, doi: 10.1175/2011JTECHA1489.1

It seems that the bias correction model presented here would currently make Met Office and ECMWF bending angle biases worse. Is that correct? Should the model be applicable to GRAS data?

This statement can only be made if we assume the bias correction procedure in the forward modeling mentioned by the Reviewer above. However, our approach does not involve any forward modeling. Our model only works with RO observations.

The bias correction model given in section 2 seems overly complicated and requires many predictors. How many predictors are used typically in the radiance bias correction schemes? Are more predictors required here? Why?

Radiance bias correction is beyond the scope of our paper. Our bias correction model is very fast: we can hardly notice any increase in the computational time; the core Fortran module is only 574 lines long (including extensive comments); in addition there is a file with 214 regression coefficients. Can we really characterize this as *overly complicated*?

Anonymous Referee #2

The manuscript is interesting and follows in general appropriate logic flow. It requires however some improvements, as detailed below. I recommend minor revision. I encourage otherwise the authors to work to clarify the text, as it is at times difficult to follow.
My main concerns are presently not addressed in the paper, but could be addressed with better justification, explanation, or reference to external material. These are the following two items:
1) The authors show that fluctuations following the structure function presented in Fig i produce a negative bias that is very similar to the one generally known to exist. That structure function is not unreasonable, but the authors do not present a link between known or expected atmospheric properties of turbulence, or temperature and moisture fluctuations, and the ${C_N}^2(z)$ *presented. Why that profile of fluctuations? A later sentence (P4L9) says "refractivity fluctuations can explain and quite well describe the systematic and random error...". The agreement found actually means that some fluctuation profile can be found that reproduces the known bias, although it has not been shown or referenced whether that profile was realistic*

at all. Beyond, the $C_N^{\,2}(z)$ shown is peaked at the low troposphere, descends near the surface, and also monotonically reduces above the PBL. A realistic $C_N^{\,2}(z)$ may have also other minor peaks and features.

We agree that these points need clarification in the paper, and some of the current formulations can be misleading for a reader. As we already stated above, in the rebuttal to Reviewer #1 comments, this model is a good structural model that allows finding good candidates for the bias predictors. All the bias estimates are based on the objective characteristics of the signal received. As discussed in more detail, in the paper by Gorbunov, Vorobiev, and Lauritsen (2015), that, with the corresponding choice of the effective profile of $C_N^{\,2}(z)$, the fluctuation model can reproduce the statistical characteristics of the observed bias. However, because this model is not directly used in the bias correction procedure, the further discussion of $C_N^{\,2}(z)$ is beyond the scope of this paper. We updated the formulations in the paper along the lines of this discussion. In particular, we added the following remark to the discussion of Figures 4 and 5:
"An important conclusion from these comparisons is that the fluctuation model alone does not explain the patterns observed in the real observations. However, the role of this model is to help finding reasonable predictors. The further bias correction procedure is only based on the predictors that can be readily derived from observations, rather than on the fluctuation model."

2) Although the idea of estimating the expected bias through an atmosphere of given fluctuation properties is interesting, the proposed solution is an empirical regression, where the bias (wrt ECMWF) is reduced. I am concerned about the impact on traceability, since the lower bias is obtained by heuristic fit, rather than by a physical link. Among other concerns, it simply succeeds on reproducing the bias of ECMWF (which may itself be biased) with a large number of predictors. This is the procedure normally applied to, for instance, radiance measurements. Historically, one of the major benefits of radio occultations has been the possibility to use these data without such heuristic bias correction. Otherwise, the number of predictors and *adaptive functions being so large, it would have been surprising not to be able to fit the bias. A bias reduction with a very small number of predictors, and more physically based, would be more solid.*

Historically, there was a belief that RO data were not biased. However, the further development indicated that these data were indeed biased. Currently, there is no a good physical model that can qualitatively describe the bias. We can only say that the observed bias is a multi-factor phenomenon. In this paper, we are discussing an empirical approach to the estimate of the bias from well-defined predictors derived from objective characteristics of measurements. It is true that still we need some reference, and if we use ECMWF data, we involve the bias immanent to ECMWF. On the other hand, the method itself will stay, if we include some independent bias estimate of ECMWF. We added some remarks along these lines to the Conclusions.

Several minor details follow.
P8L8: "energy density of rays". Please define the meaning here of "energy density".
More precise formulation is “the normalized the energy distribution over rays in the impact parameter space.” For more details, see the references (Gorbunov, 2002; Gorbunov, 2004).

P10L11: Given those many predictors, one question that arises is why this set? Why not others, such as season, topography, land/ocean?
Season was tested and found to be a weak predictor. Topography and land/ocean may be worth further investigating, although, as suggested by the new Fig. 8, they are not unambiguous.

P10L18: "limiting the adaptive functions to the reasonable ones" What is the meaning of "reasonable" here?
We agree that the word “reasonable” has no precise definition in the context. We re-phrased this as follows: “… apply some additional constraints in order to reduce the number of adaptive functions.”

P14L26: "reasonable profiles of $C_N^{\ 2}(z)$*". It has not been justified that these are reasonable. Only that they would reproduce the bias.*
Yes, as already discussed in the previous responses.

Figures 5 and 6: Is it my perception or the procedure is moderately overcorrecting
To some extent, they are. However, The data processing chain with the uncertainty propagation requires the back projection of bending angle bias to the excess phase and amplitude.